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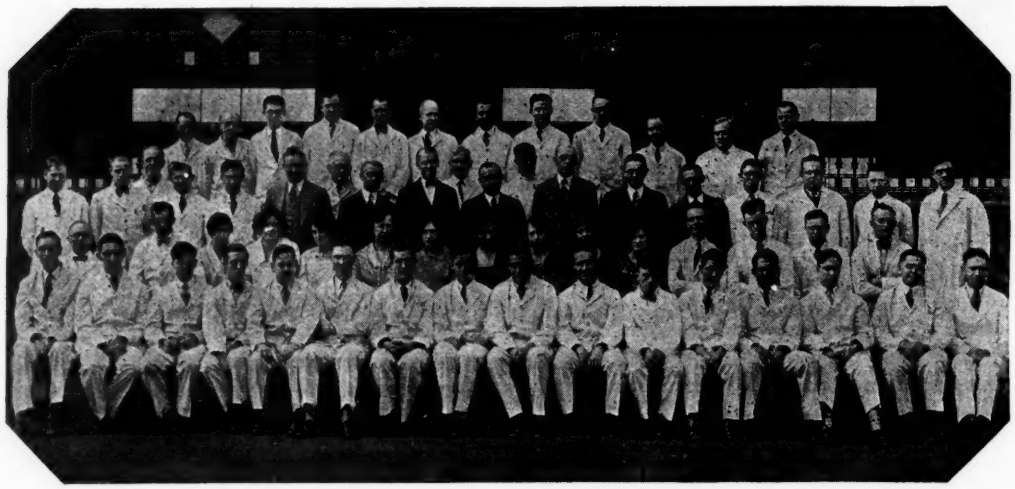

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ORIGINAL ARTICLES

THE SIGNIFICANCE OF HEMOLYTIC STREPTOCOCCUS IN THROATS OF HOSPITAL PATIENTS.*

By

DR. D. L. RICHARDSON, PROVIDENCE, R. I.
MR. EDWIN KNIGHTS, OF CITY HOSPITAL STAFF, COLLABORATING.

About 50% of those who have investigated the cause of scarlet fever concluded that streptococci either were the only etiological factor or at least played a prominent role in its causation in conjunction with some unknown organism. This belief was so widespread that when Dr. and Mrs. Dick first began in 1921 to publish the results of their experiments with hemolytic streptococci they were read by sympathetic listeners everywhere. The human experiments which they performed and which were done in a scientific manner, were very convincing and, while there are hiati yet to be filled in, authorities generally not only in this country but all over the world are satisfied that scarlet fever is caused by some strain or strains of hemolytic streptococci.

Up to the present time there has not been found any reliably scientific method of differentiating the scarlet fever hemolytic streptococci from other strains of hemolytic streptococci in spite of extensive investigations. A technique frequently employed for this purpose is to identify the toxin produced by the hemolytic streptococci under investigation by skin tests and these, as is well known, are not always to be depended on. Other methods of differentiation are employed but they certainly have not been perfected.

The valuable contributions of Dr. Ruth Tunnicliff are an excellent example of the progress being made in the identification of scarlet fever streptococci. It is quite obvious, however, that any technique which depends on the use of immune sera presupposes that animals have been immunized to all strains of streptococci which will produce the disease in human beings. The Dicks have definitely shown that there are at least two culturally different strains of scarlet fever streptococci, and any polyvalent immune streptococcus serum may still lack antibodies of one or more specific strains. Continued research and human experimentation may be depended upon to in time obviate this difficulty. If one is to be very critical only those strains which were used by the Dicks in their human experiments can be claimed as being scarlet fever hemolytic streptococci. And yet, both scarlet fever toxin and antitoxin are being prepared from other streptococci found in the throats of scarlet fever patients. Even the Dicks approve of the use of certain strains of streptococci isolated from scarlet fever patients which have not been used to produce experimental scarlet fever, along with their two original proven strains in the preparation of these two products. While undoubtedly most of these other strains are authentic strains there is sufficient chance of error to cast some doubt on the reliability of these products. In spite of this, however, there is sufficient data from clinical experience to warrant the use of scarlet fever antitoxin in selected cases, those which actually need it. Less faith can be placed on the results of the toxin for the Dick test. On the other hand, there are many favorable reports on the reliability of immunity produced against scarlet fever by toxin inoculation.

Naturally, following the discovery of the Dicks, physicians became intensely interested in the possibility of culturing the throats of patients as an aid to diagnosis and to determine when a scarlet fever patient could with safety be discharged from isolation instead of relying on an arbitrary period of quarantine. In other words, to follow procedures which have long been used in diphtheria.

Over three years ago at the Providence City Hospital we began taking cultures of scarlet fever patients at the time of discharge after the

*Read before the Annual Meeting of the Rhode Island Medical Society, June 6th, 1929.

usual isolation period, with the idea of discovering whether patients who went home with hemolytic streptococci in the throat were more likely to give the disease to other members of the family than those who had no organisms at the time of their dismissal. Previous to 1927 this practice was not uniformly carried out and since it is important to draw conclusions only from a large number of cases publication of our experience is deferred to a later date.

So impressed had we been with the possible value of knowing whether patients had hemolytic streptococci in their throats on admission as an aid to diagnosis and on discharge for detecting carriers, that in Februray 1927, a technique was introduced for taking cultures on all new patients routinely and at discharge on all scarlet fever and diphtheria patients.

In the course of this work we have sought to throw a little more light on four points in particular.

1st. Disregarding cultural and serological classifications of the hemolytic streptococci (Beta type) we have attempted to find out of what value, if any, blood agar plate cultures of throat and nose secretions would be as an aid in diagnosis.

2nd. The incidence of hemolytic streptococci (Beta type) in the major contagious disease admitted to this hospital.

3rd. The value of blood agar plate cultures in determining the necessary quarantine period in scarlet fever.

4th. And last but not least the practicability of substituting blood infusion agar for Loeffler's coagulated blood serum as a routine nose and throat culture medium.

During the period 1910 to 1927, inclusive this hospital followed the example of practically all other contagious disease hospitals in using Loeffler's coagulated blood serum as a medium for culturing diphtheria bacilli. It seemed like breaking a sacred laboratory rite to think of letting a standard medium of proven worth like Loeffler's go by the board, but that is exactly what was done. It was not possible to take duplicate cultures on Loeffler's serum and on blood agar every time a nose and throat culture was wanted. The volume of cultural work would have been too great.

It became necessary, therefore, to prove the value of blood agar as a medium for diphtheria cultures. This was done by running duplicate cultures during January, 1928, on all the patients in the diphtheria ward. The results were even better than we had expected. Not a single positive diphtheria case was missed culturally on the blood agar that was found positive on the Loeffler's medium, and a few positive cultures were picked up by the blood agar which were negative with Loeffler's medium.

The question of expense was next considered and here again a point was gained in favor of the blood agar plate method. Liter for liter the actual cost of the ingredients of the blood agar medium was much less than the cost of the ingredients of Loeffler's medium. This may in part be due to the fact that sheep's blood taken with sterile precautions at the time the sheep are bled to furnish blood for the Wasserman test, was used.

The labor involved in the preparation of the blood agar plates is insignificant in comparison with the labor involved in the preparation of Loeffler's medium. In one day devoted to the preparation of stock infusion agar (the base for the blood agar plates) one man can prepare 30 or 40 liters of infusion agar, adjust the entire batch to a uniform P. H. (7.8) and sterilize it in convenient amounts for pouring plates. Such an amount of stock medium lasts us from 3 to 5 months and besides its convenience provides the added advantage of a uniformity in product which it was never possible to obtain with Loeffler's medium.

Standard glass covered petri plates 3½" in diameter receive approximately 10 cc of the 7½% of sheep's blood infusion agar. A smaller size dish could well be used for further economy. On one-half of each plate is planted the throat culture and on the opposite half the nose culture. This eliminates sorting and pairing up of nose and throat cultures on the same patients when the cultures are smeared for examination. After 12 to 14 hours incubation the cultures are smeared from the plates on glass slides, stained with a suitable dye and examined microscopically for diphtheria bacilli. The same plates are also examined microscopically for colonies of hemolytic streptococci and their prevalence is indicated in the report in degrees of 1, 2, 3 and 4 plus.

Very little difficulty is experienced in recording these results, and with a little practice it was found unnecessary to transplant hemolytic colonies to broth for further identification. When we speak of hemolytic streptococci we refer to the broad classification which Schottmueller (Schottmueller, Munden, Med. Wchuseter., 1903) describes as streptococcus longus or hemolyticus and which has been more recently elaborated upon by Smith and Brown (Smith & Brown med. research, 1914, 31:455 Monogr. Rockefeller Institute, January, 1919, No. 9) as the Beta type of hemolytic streptococci.

No doubt there has been a small percentage of error in the cultures which were classed as 1 and 2 plus positive for hemolytic streptococci but the 3 plus and 4 plus plates were extremely easy to distinguish. In the data compiled below the 3 plus and 4 plus cultures have been put in one group and the 1 plus and 2 plus cultures in a separate group to help make allowance for these errors.

During the period February 1, 1928 to January 31, 1929, inclusive 1794 patients were admitted to the Hospital and during the same period the laboratory examined 11,838 cultures for hemolytic streptococci on these patients. For the purposes of this study 8,678 cultures, taken on 1,407 of these admissions have been tabulated. The 387 admissions not tabulated include a certain number of patients still in the hospital at the time the tabulation was started and a large number of syphilitic cases admitted for lumbar punctures only and from whom no cultures were obtained.

Grouping the cases of contagious diseases admitted during the period of this study by the primary diagnosis on discharge, the number of cases in each group and the number of cultures taken are shown in table I.

TABLE *I.*

	Number of patients	*Number of cultures*
Scarlet Fever	335	2960
Measles	287	1210
Diphtheria	216	2102
Naso-pharyngitis, Laryngitis	75	398
Influenza	72	282
Tonsillitis	62	374
Whooping Cough	47	104
Erysipelas	35	142
Other Diseases, Tuberculosis, Gonorrhoea, Syphilis, Bronchitis, Pneumonia, Chickenpox, Otitis Media, Mumps	278	1106
	1407	8678

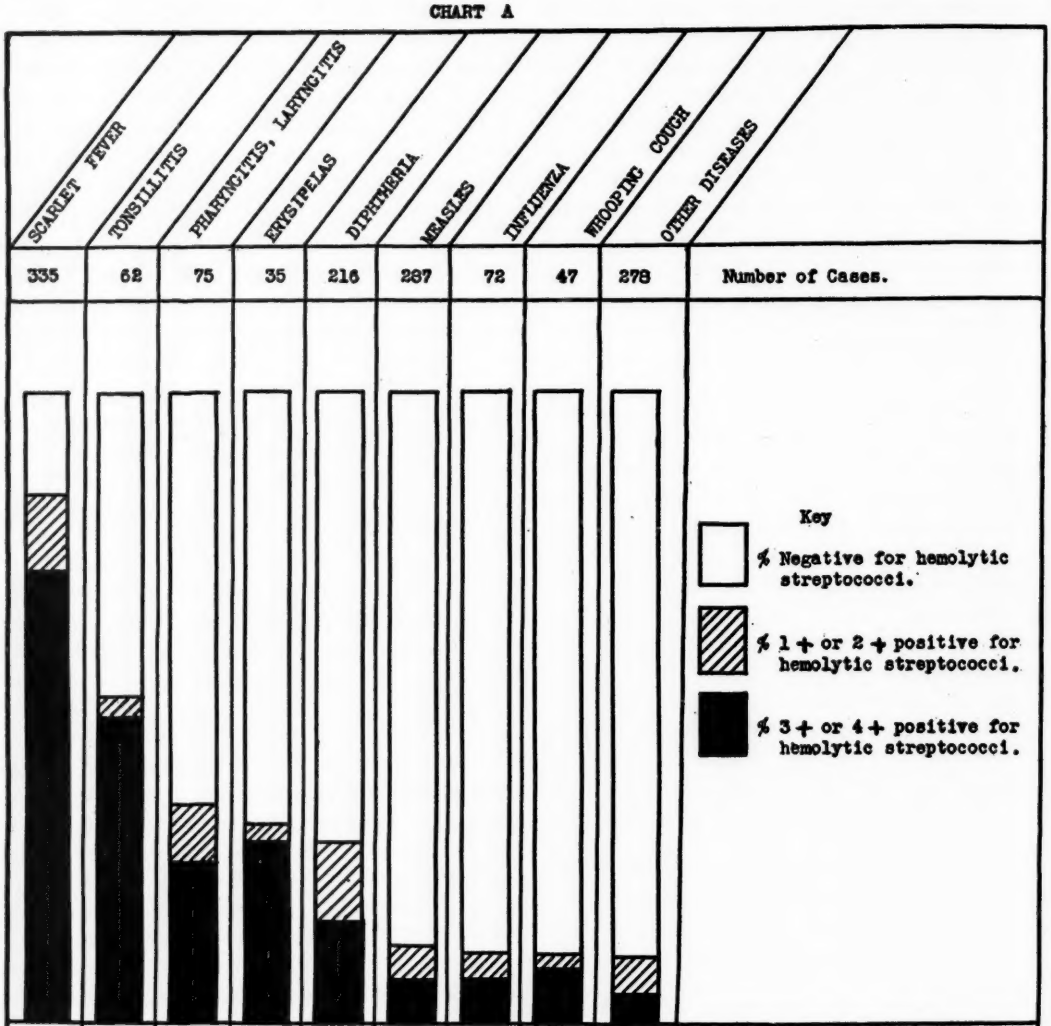
A thorough study of the 1,401 cases included in Table I was made and much interesting data obtained. Chart "A" shows the results of all of the cultures taken in the various disease groups, and in each disease it shows the percentage of cases which showed 3 or 4 plus cultures, the percentage of cases which showed only a 1 or 2 plus and the percentage of cases which had negative cultures for hemolytic streptococci.

A study was next made of the percentage of cases in the various diseases which showed a 3 plus or a 4 plus hemolytic streptococci culture in one or both of the first two cultures, taken on admission. A chart prepared from these figures (Chart B) could, for practical purposes, be superimposed on (Chart A). This demonstrates that the percentage of cases which showed 3 plus or a 4 plus positive cultures at any time while in the hospital was practically the same as the percentage in which either the first or second culture taken was 3 or 4 plus positive.

Data concerning the percentage of cases of scarlet fever showing positive hemolytic streptococci at the time of discharge is readily obtainable from our records. During the period of this study the percentage of discharged scarlet fever patients who showed positive hemolytic streptococci cultures at the time of discharge was 28.06% and 71.64% were negative.

For the same period a study was made of the prevalence of diphtheria carriers among admitted patients. Among 1,190 patients admitted for diseases other than diphtheria 8.2% were diphtheria carriers. During the same period about 11% were hemolytic streptococci carriers excluding scarlet fever, tonsillitis, naso-pharyngitis and laryngitis, erysipelas and diphtheria. It is obvious that the incidence of scarlet fever carriers was about the same as that of diphtheria carriers. The high incidence of hemolytic streptococci in

CHART A

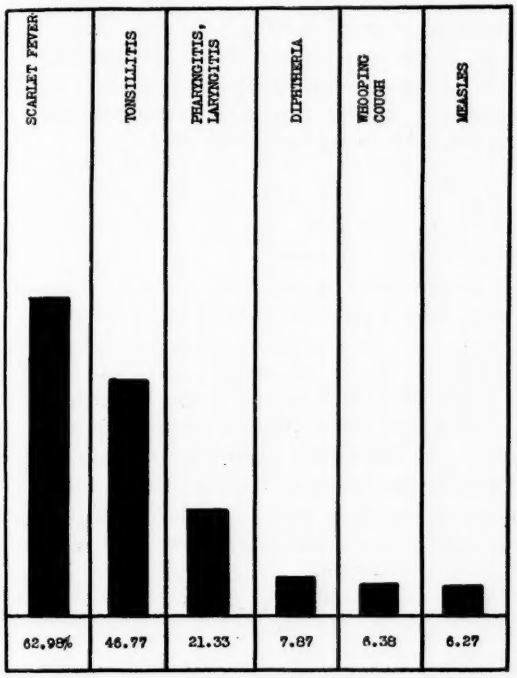


SCARLET FEVER	TONSILLITIS	PHARYNGITIS, LARYNGITIS	ERYSIPELAS	DIPHTHERIA	MEASLES	INFLUENZA	WHOOPING COUGH	OTHER DISEASES	
335	62	75	35	216	287	72	47	278	Number of Cases.
240	30	19	10	34	20	5	4	12	Number Of cases 3+ or 4 +
71.64	48.38	25.33	28.57	15.74	6.96	6.94	8.51	4.31	% " " 3 +or 4 +
39	2	7	1	28	16	3	1	17	Number of cases 1 + or 2 +
11.64	3.22	9.33	2.85	12.96	5.57	4.16	2.13	6.11	% " " 1 + or 2 +
279	32	26	11	62	36	8	5	29	Total cases 1,2,3,or 4 +
83.28	51.60	34.66	31.42	28.70	12.53	11.10	10.64	10.42	% of " 1,2,3,or 4 +
56	30	49	24	154	251	64	42	249	Number of cases Negative .
16.71	48.38	65.33	68.57	71.29	87.45	88.88	89.36	89.56	% " " " .

acute diseases of the nose and throat can be explained by the fact that some of these were actually suffering from scarlet fever without the eruption, and that streptococci have long been known to be associated with acute throat conditions. Erysipelas is known to be caused by some strain or strains of streptococci. That the percentage of streptococci carriers is high among diphtheria patients may be explained in part by the fact that some of these patients had scarlet fever rather than diphtheria and should be classed as scarlet fever patients who were diph-

CHART B

PERCENTAGE OF CASES IN VARIOUS DISEASES WHICH SHOWED A 3+ OR 4+ POSITIVE IN EITHER ONE OF THE FIRST TWO CULTURES TAKEN.



theria carriers rather than diphtheria patients who were hemolytic streptococci carriers. Every year there are admitted a considerable number of acute throat conditions which cannot be classified clinically. These are usually called diphtheria if the cultures are positive and we can't explain the sore throat in any other way. In further explanation of the fact that the hemolytic streptococci carrier incidence among diphtheria patients is high it may be noted that the diphtheria carrier incidence among scarlet fever patients is almost always higher than the carrier incidence among other patients. Still another factor to bear in mind is that by the technique employed the finding of hemolytic streptococci carriers is probably more accurate than that of diphtheria carriers.

From facts which have been presented definite conclusions cannot be drawn but certain observations can be made.

1. When hemolytic streptococci are found in the throats of patients suspected of having scarlet fever this helps to confirm the diagnosis providing at least two or three cultures have been taken. If an equal number of cultures are negative this is even stronger evidence against the diagnosis of scarlet fever.

2. That, after ruling out tonsillitis, naso-pharyngitis and laryngitis, erysipelas and diphtheria, diseases which are either caused by streptococci or often associated with these organisms, the carrier incidence is only slightly higher than that of diphtheria.

3. That, quite likely epidemics of tonsillitis, otitis media, upper respiratory infections, and of infected wounds often seen in general hospitals are started by over-looked streptococci in disease particularly of the nose and throat and that some protection might be afforded to other patients by isolating all patients in whose throats hemolytic streptococci are found. The hemolytic streptococcus is one of the most deadly of all disease producing organisms and while there are mild strains of these organisms they so frequently cause serious infection or death, that whenever these germs are found in the nose and throat or in wounds the patient should be isloated.

4. That, blood agar plates are entirely satisfactory for culturing diphtheria bacilli as well as hemolytic streptococci and that it saves time and expense to use the one cultural procedure for both organisms.

DR. JORDAN: "A case of scarlet fever without eruption may be considered as one of severe tonsillitis until some other member of the family presents a rash.

"Many cases of sore throats with 4-plus hemolytic streptococcus are undoubtedly scarlet fever without a rash."

"Hemolytic streptococcus is always a bad organism to have in the throat."

DR. CHAPIN: "Carriers are more frequent than was supposed. It makes us wonder if we are accomplishing much by placarding a house when so many carriers are at liberty."

DR. MCLEOD: "A child with glands and history of running ear:—Culture showed hemolytic streptococcus. Quarantine was followed by a cessation of scarlet fever."

DR. BRACKETT: "Numerous cases of hemolytic streptococcus, post-operative wound infections:— Operating room nurse who was found to be a carrier read aloud before petri dishes. Numerous hemolytic streptococci were found. The performance was repeated with a mask over the mouth and the number of germs was found to be decreased. No germs were found at all when both nose and mouth were covered by a mask. She continued to serve in operating room wearing nose and mouth mask and further infection did not occur."

DR. RICHARDSON: "A sore throat that can not be otherwise explained with 3 or 4 plus hemolytic streptococcus should be isolated, especially if there is vomiting.

The history of positive contact with scarlet fever should be persistently sought for.

A white count over 15,000 and eosinophilia, indicates scarlet fever.

There have been a number of return cases from discharged patients whose throats on discharge were negative as well as some whose throats showed 1 or 2 plus hemolytic streptococcus. This leaves us in a state of uncertainty as to the significance of the presence or absence of hemolytic streptococci in the throats of discharged patients.

CASE REPORT*

PRESENTED BY
DR. WILLIAM P. BUFFUM,
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Providence, R. I.

Joseph P., age 10, birthday January 11, 1919.

In 1924, he had his tonsils removed because of frequent sore throats. No mention of rheumatic fever was made at this time.

In December, 1925, at 6 years of age, he was admitted to the R. I. Hospital with rheumatic fever and a diagnosis of heart disease was made at this time. He was discharged after one month and came to the O.P.D. At this time he looked sick, and his pulse was 120 sitting. His heart was enlarged, the left border being 3 cm outside the nipple, and the right border 4 cm from the midsternal line. The sounds had a tick tack rhythm and there was a slight systolic murmer at the apex. There was also a definite soreness in one ankle. His weight was 45 lbs. During 5 months we had almost no control over him and only saw him twice. His condition remained about the same until May, when he developed more arthritis and was readmitted to the R. I. Hospital. After 2 months in the hospital, he was one month at Crawford Allen and returned to the O.P.D. in August, 1926. The physical examination at this time was almost exactly the same and he had pain in both ankles.

Over a period of 8 months since we had first seen him in the O.P.D., he had had constantly arthritis, rapid pulse, and poor heart action. He had lost 3 lbs. in weight.

Joe was then sent to the Lakeside Preventorium and left there 6 months. For 6 weeks he was kept in bed all the time and then he improved so much that he was allowed up and about. On discharge his general condition was excellent. His pulse was less than 100 and reacted well to exercise. His heart was slightly smaller with a loud systolic murmer and a slight systolic thrill. It was at this time that Dr. Robey demonstrated him here, at the meeting of the New England Heart Association.

During the following year his condition was not so good. He lost 1½ lbs. in weight and finally developed arthritis and chorea.

Just a year ago he was sent to Lakeside again We kept him in bed constantly for four weeks and since then his exercise has been increased very gradually. For the last 2 months he has been allowed to play with the other children. He gained 11 lbs. and is now in good general condition. His heart borders are 2 cm to the left of the nipple and 5 cm to the right of the midsternal line and the other signs are typical of mitral stenosis.

This case is presented because it shows clearly the need of more careful supervision of children convalescent from rheumatic carditis. During the period from January to August, 1926, this boy was suffering from a subacute rheumatic infection with one acute recurrence. During most of the time he was not well controlled by his parents, and except for a short stay in the hospital was not kept in bed. Obviously there should be a cardiac convalescent home or some existing institution should adapt itself to take care of these cases.

*Read before the Providence Medical Society, April 1st, 1929.

CANCER RESEARCH PAST AND PRESENT*

By Herman C. Pitts, M.D.
Providence, R. I.

From the moment man became a thinking personality, perhaps only a little raised above the beasts that perish, he began to speculate on the causes of life and death and to cast about for means to cure or relieve the curse of illness that came upon him. Its cause was most naturally laid to evil spirits or to the displeasure of some god and the cure in consequence depended on propitiation of these. Whether man in these earliest of days made any attempt to classify disease, we have no means of knowing. Probably not. Later on, much later, in ancient India, in what is now Asia Minor and in Egypt, medicine became an art and the practice of that art in the hands of a few, pre-supposes an increased knowledge of disease. The fragments of writing that exist to the present day are interesting but not instructive. The first real start in scientific medicine was made in the Golden days of Greece, in the age of Pericles and the man who made the start and whose influence was felt for hundreds of years, was Hippocrates. His clinical notes after 2,500 years give us a picture of disease that cannot be surpassed. He describes cancer and gives as a cause an accumulation of black bile in the area affected. So here we have in Hippocrates' writings, the first recorded speculation, research as it were, into the nature of the disease we are considering. Even in his day, he found that art is long, experience is baffling and cure difficult. And so it has been down through the ages. Excision where excision could be practiced and a vegetable diet for internal tumors, was what Hippocrates recommended. How very little we have progressed since his time.

The pitiful inadequacy of such treatment led to further study, as such study became possible through increased knowledge; and further study led to the formation of various theories as to the nature and cause of cancer.

I trust it will be of interest to review briefly these theories and their rise and fall, and to give some idea of the enormous amount of time and treasure that have been devoted to cancer research. That this search is baffling is proved by the fact that to the present time nothing we have learned brings us any nearer to a solution of the problem. Much has been learned—but much more must still be learned before the key is found that will unlock the casket that hides the mystery.

Among the many theories regarding cancer that have occupied men's minds and through which they have sought to explain its cause and control and its growth, the embryonal theory of Cohnheim stands pre-eminent. This is so because the known facts fit in so exactly with what it holds. The theory was outlined in fact, many years before Cohnheim elaborated it, when Lobstein in 1829 pointed out the similarity between the growth of embryonal tissue and cancer. The difference between the two, lying in the fact that cancer had lost the controlling influence of the organism in which it grew. Remak in 1854 maintained that cancer developed in misplaced islands of epithelial cells existing in tissues not normally containing epithelium. These two earlier ideas were elaborated by Cohnheim into a perfectly logical and well worked out theory. Careful study revealed the fact that a long list of tumors, by no means all of which are malignant, must arise from small bits of misplaced embryonal tissue. The extreme is reached when we consider dermoid tumors with their collection of hair and teeth and bone. It has been found that these embryonal rests are really common, that each one of us probably harbors many such minute collections of misplaced cells. The weakness of the theory lies in the very fact that the majority of human beings live a long life and die without developing cancer. They tell us one in eight women and one in eleven men die of cancer. Terrible as that mortality is, there is still a large percentage in which the embryonal rests live on in perfect content and never become imbued with ideas of expansion. Evidently something more than Cohnheim's theory is needed to explain the existence of cancer.

Anyone of us who thinks continuously on any subject gradually comes to hold very definite ideas concerning it. So it was that about ten years ago I out of my own feeble intellect, evolved what I thought was a most wonderful theory—one so simple that I coudn't see why it hadn't been thought of before. Imagine my chagrin and

*Read before the Rhode Island Medical Society, March 7th, 1929.

the pricking of my toy balloon when I read that the germ of my theory, its very foundation, had been put forward nearly one hundred years ago! Nevertheless, as it is perfectly logical and since it deals with cells and is more or less linked with Cohnheim's theory, I should like to present it here—but not as my own. The theory is based on the control over cell growth exercised by the organization of the body. We can readily grant that as the embryo develops and certain cells become differentiated into epithelial cells, or liver cells or what not, they assume a certain definite structure and that in their dividing, the resulting cells, if the division be normal, will have the same differentiating structure. They inherit this structure from generation to generation and as long as their particular structure is maintained they will behave as epithelial cells or liver cells or whatever type they may represent and will be under the control of the organism. Now suppose, because of a change in nutrition or a change in the balance of salts supplied, a parent cell in its division fails to transmit its full differentiation. The daughter cells are slightly changed and their progeny deviate still more from the normal. Very soon, we have a collection of cells that are dangerously different from the orderly group from which they sprang. They have a potential capacity for growth, they are free from the hampering control of the organism and the stage is very logically set for the wild overgrowth of cells that we call cancer. It all sounds very reasonable but it does not as yet anyway explain what starts that first cell going wrong.

The parasitic or infectious theory of the origin of cancer is one of the very oldest. It dates back to ancient times, lived on through the Middle Ages and today, is strongly advocated by many. It had its origin in the observed fact that cancer appeared in several members of a household or was prevalent in certain localities or even in certain houses. These cancer houses, so-called, were tabulated and the number of cancer cases appearing in them over a course of years counted up. The results convinced many that these houses must harbor a contagion of some kind that passed the disease on from person to person. Many have argued that an infection must come through contaminated water or soil. Of course, there are strong arguments against the theory. If cancer were contagious, it is almost certain that doctors and nurses who come in contact with the disease so often would show a much larger cancer incidence than any other group. This we know is not true. Then again, it is pointed out that husbands of wives suffering from cancer of the uterus do not develop cancer and that wives whose husbands have cancer of the penis are no more afflicted with cancer than the average.

With the advent of the microscope came a tremendous search for bacteria and protozoa, spirochetae and blastomycetes as the cause of cancer. No one of the great numbers found and ascribed as the cause has stood the test of time. The expenditure of labor and time and money in this very fruitless search has been astonishing.

When Rous, in 1911, announced that a filtered extract of a very virulent chicken sarcoma injected into healthy chickens produced a similar growth, the scientific world felt that at last progress had been made; that his experiments proved that in sarcoma at least, an ultra-microscopic organism, one that would pass through a fine filter, must be the cause. Further careful study seems to show that this fitrate must be classed as a chemical irritant and the production of a tumor by it in the words of Prof. Ewing "merely illustrates the indirect action of irritants on pre-disposed tissues." Since Rous' time, the most noteworthy contribution along these lines has come from England. In July, 1925, Gye advanced the theory that all tumors are due first to a non-filterable, ultra-microscopic virus common to all tumors, and second, to an unstable chemical agent, peculiar to each tumor and also to each tissue. This he calls the "specifice factor." For instance, according to his theory, the virus from a mouse carcinoma plus the specific factor from a fowl sarcoma, injected into a healthy fowl will produce sarcoma. The virus according to Gye could be cultivated anaerobically. The specific factor resides in a sand and paper filter of the tumor tissue studied, treated with enough chloroform to kill whatever virus it might contain.

The whole theory is most ingenious but it has one very grave and very damaging fault. No one besides Gye, using Gye's own technique has been able to get Gye's results. And even if they could, even if we grant that Gye's argument is correct, we are still at a loss to explain cancer. What is the "specific factor?" Are we to suppose it is present in each one of our numerous dif-

ferent tissues at all times waiting for the virus to enter and when it comes, joining hands with it to produce a tumor? It sounds absurd surely. The solution of the problem is not yet!

There is no belief regarding cancer more firmly intrenched than the belief in its being hereditary. And I must say, a study of the histories of patients gives good grounds for that belief. In the literature, we find numberless histories of families where a majority of the members through several generations have been sufferers. The experience of each one of us is so bound to confirm the general feeling that it is hardly necessary to give further evidence. One celebrated case will suffice; Napoleon, his father, one brother and two sisters are supposed to have died of cancer of the stomach.

Experiments in breeding mice have shown without question that the percentage of cancer in certain strains can be greatly increased by proper in-breeding. Maud Slye who has done an enormous amount of work along these lines believes that cancer is inherited after the manner of a Mendelian recessive. That is, if a mouse from a cancerous strain is bred to one from a non-cancerous strain—the progeny do not develop cancer, but are capable of transmitting the tendency to cancer to the next generation. Slye wishes to apply what she has learned in the laboratory, to human beings and believes that by proper eugenics, a tendency to cancer in man could be bred out entirely.

There are many who believe that Slye is wrong, that her experiments prove nothing and that her deductions from her experiments which prove nothing, are absurd. So there we are again—left as usual with nothing definite to pin faith to! If Slye is right, then surely the dictum that in the interests of the public this doctrine should be combated is wrong. Rather, we should shout from the housetops that no children should in the future be born from parents either of whom gives a history of cancer in the family.

There is one thing in cancer genesis that seems fairly clear and that most researchers can agree upon and that is the influence of trauma. This may, of course, be of many kinds—such as mechanical, thermal or chemical. The mechanical irritation of a jagged tooth, for instance, or the celebrated and off quoted Kangri cancer, due to the repeated irritation of the abdomen by the hot charcoal warming pans worn on the abdomens of men in mountain India, or the chemical irritation of aniline excreted in the urine, in producing cancer of the bladder in aniline workers, are all examples. A knowledge of these facts has caused an enormous amount of experimental work on animals. Countless rabbits have had their backs and ears painted with coal-tar over long periods of time and the epithelial growths resulting occasionally after these efforts have been studied. The fact that some of these animals fail to grow cancer—that many humans may suffer trauma of some kind for years without cancer developing points pretty clearly to there being another very elusive element necessary. The trauma, whatever it may be, must be considered the exciting cause and not the whole cause. In other words, the potential tendencies lying dormant in the individual's tissues are merely brought to light by the trauma.

I am led to speak of and express my admiration for the marvelous work on tissue culture that has been done in an effort to explain cancer. When one sees the incubators at the Rockefeller Institute crowded with bits of tissue of all kinds growing in an artificial medium, one marvels at the ingenuity of man. And yet, I must confess that the work leaves me cold. At an enormous cost of money and time and skill, they prove one thing and one only; and that is that almost any group of cells can exist on artificial food when separated from the mass of its fellows. There is only one chance that great good can come from experiments along this line. If some day, through special feeding, normal cells can be made to develop into cancer cells, the world has learned a secret that will be of the greatest value.

When all is said and done, it seems to me that a solution of the cancer problem lies in the study of the cancerous individual as compared with the probable normal. In our cancer work in the Brown University Laboratory, we are following this line of attack.

Cancer patients as they are admitted to our cancer clinic are studied as systematically as possible and the results obtained are compared with those from the normal or at least non-cancerous individuals. As we go along, new methods of study of course suggest themselves. Until a very

large series can be tabulated and comparisons made, it is perfectly useless to speak of our results. In conjunction with our work on patients, we are carrying on quite extensive work with cancer in mice. Through the kindness of Dr. Boyd, we have been able to treat a long series with X-ray after the tumors have been injected with various metallic salts. I am sorry to say our results were disappointing. At present, we are trying the injection of calcium chloride in various strengths directly into the tumors. Quite a fair proportion of these tumors gradually disappear and the mouse remains well. I am not as enthusiastic as I might be over these results because these tumors being transplants are more or less encapsulated and their disappearance is probably due in large measure to the killing action of the calcium chloride solution on the cells in a confined space.

As soon as we have bred a few rats and have tumors growing in them, we expect to study the effects of lead on tumor growth. We are the proud possessors of some radio-active lead. It's action may not differ, of course, from the action of ordinary lead. On the other hand, it may. At any rate, we will watch the results with much interest.

We are probably presumptious in attacking this problem at all. In fact, I have been told as much by one gentleman whom I approached for money to carry on the work. "If they have found out nothing at Rockefeller Institute or at the Crocker Institute or at Huntington Memorial or at any other of the many well equipped places devoting all their time and brains and energy to the work, what chance have you people here," he said, "to add even one little bit toward the solution of the problem." Very likely he is right and we have no chance and yet, I am buoyed up and urged onward by what the head of that very Crocker Institute, Dr. Carter Wood, said in a talk last year. His words were very nearly these; "Up to the present, we have found nothing. We seem to be as much in the dark as we were fifteen years ago. I believe the solution of the problem will come. It may not come in my life time—on the other hand, it may come tomorrow. Where it is coming from—no one knows. The obscure worker in the small laboratory has as much chance as the worker in the best equipped laboratory in the world." He didn't mention Providence specifically, you see, but the "obscure worker" and the "small laboratory" describe us exactly, and so we are encouraged to strive on.

STUDIES ON DIGITALIS IN AMBULATORY CARDIAC PATIENTS

Harry Gold and Arthur C. DeGraff, New York (*Journal A. M. A.*, April 27, 1929), in making studies on digitalis in ambulatory cardiac patients found that regarding the use of digitalis it is essential to bear in mind the practical distinction between (1) types of failing circulation in which the use of the drug results in striking improvement, and (2) types of failing circulation in which the use of digitalis is indicated on the basis of certain experimental data and theoretical considerations, but in which clinical study thus far gives evidence of little, if any, beneficial effect. It is pointed out that numerous errors in the interpretation of clinical observations have arisen from failure to consider this distinction. There is no essential difference between the behavior of digitalis in children and in adults. The drug is less often seen to produce striking improvement in children than in adults because the type of heart failure that is relieved most effectively by digitalis (congestive heart failure without active infection of the heart) is relatively common in heart disease among adults but relatively rare in that among children. In those cases in which less definite therapeutic effects are obtained, insufficient or excessive digitalization is more apt to occur because of the absence of a satisfactory guide to the intensity of digitalis action. Digitalis cumulation, as occurring in the course of the daily administration of a suitable fixed dose of the drug, can be shown to be a self-limiting process. The intensity of digitalis action present at the time when further cumulation ceases to occur depends on the size of the daily dose. Experiments cited show that, contrary to the view commonly held, a patient does not eliminate a fixed quantity of digitalis daily, but a quantity that varies with the amount present in the body. It is shown that the full therapeutic effects of digitalis may be induced in many cases by the daily repetition of such doses of the drug as the patient may eliminate daily after having been fully digitalized.

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EDITORIALS

THE ART OF MEDICINE

The young doctor of to-day, fresh from medical school and hospital, is wonderfully well trained in accurate methods of diagnosis. To the older procedures of history, physical examination and crude laboratory tests he adds studies in the chemistry of the body fluids, electrocardiography, basal metabolism, roentgenology and a steadily increasing number of precise examinations. In treatment, too, he has weapons of which his forbears never dreamed—synthetic drugs, biological products and physical modes which are undergoing constant modification and improvement. He has, in brief, an excellent grounding in what is called "the science of medicine."

As he proceeds to use these methods in diagnosing and treating actual patients, however, he soon learns that the practice of medicine is much more complex than he had been led to believe. Despite all the refinements of diagnosis, the ailments of too many of his patients refuse to be nicely pigeonholed; and methods of treatment have an annoying way of failing when most needed, so that new ones must be devised or old ones revived. And so in time, if he is intellec-

tually honest and can learn from experience, he comes to a realization of the fact that medicine is not and never can be a science; that it may employ scientific methods but it remains an art; and that the essence of the art consists in treating the patient and not the disease.

And if this realization comes as a sort of disappointment to his ideals of scientific medicine, let him not be downcast, for he is practicing one of the most noble of arts, with surpassing opportunities for service. Let him take hope and courage from an estimate of his work such as that given by Robert Louis Stevenson, than whom no man of letters had better opportunity of judging our profession: "There are men and classes of men that stand above the common herd: the soldier, the sailor, and the shepherd not infrequently; the artist rarely; rarelier still, the clergyman; the physician almost as a rule. He is the flower (such as it is) of our civilization; and when that stage of man is done with, and only to be marvelled at in history, he will be thought to have shared as little as any in the defects of the period, and most notably exhibited the virtues of the race. Generosity he has, such as is possible to those who practice an art, never to those who drive a trade; discretion, tested by a hundred secrets; tact, tried in a thousand embarrassments; and what are more important, Herculean cheerfulness and courage. So that he brings air and cheer into the sick room, and often enough, though not so often as he wishes, brings healing."

WORLD-WIDE MEDICINE

Perhaps there is no group of men united by ties which more definitely transcend national boundaries than do those which bind the medical profession into one homogeneous whole the world over. "Narrow Chauvinism" in *Medicine* of which Osler wrote a quarter of a century ago has definitely become less and less apparent while more and more the work of the man in, for example, Sitka, Alaska is being influenced by the investigations of his colleagues in Buenos Aires or Bombay as the case may be. Agencies for the dissemination of medical knowledge and the comparison and standardization of procedures are more efficient than ever they have been heretofore. In this connection the recent Conference of the League of Nations Health Committee on Laboratory Tests for Syphilis held at Copenhagen (May 21-June 4, 1928) is of interest. A brief report of the proceedings has been published by Kahn in the *Journal American Medical Association* for August 3, 1929. Representatives of eighteen different countries met and compared their results in simultaneous tests of almost 500 known sera which were collected in the larger medical centers of Europe, such as London Paris and Berlin. The Conference came to very definite conclusions as to the value of the various tests and specific recommendations were made. The results are world-wide in their application and afford a splendid example of that type of cooperation which, if it could be more generally applied, would mean the doom of that narrow nationalism and bickering whose end result is war.

DOUBLE FECAL FISTULA OF SIX YEARS DURATION WITH COMPLETE CURE

By Panos S. Dukakis, M.D.
454 Huntington Ave.,
Boston, Mass.

The reason for reporting this case is because of the duration of the fistula and the result obtained after operation.

The patient, a female, 33 years of age, married, having three children, was referred to me by the family physician with the history that his patient was discharging a sero-purulent material from two openings on the abdominal wall for the past six years, following an operation for a pelvic abscess which was drained at that time for five weeks. During these six years patient had received electro-therapy and Alpine Lamp treatment from various sources with temporary relief, but these sinuses had persisted up to that time.

The patient was a rather nervous and apprehensive person but fairly well nourished. The physical examination was negative except for the abdominal wall which revealed a very ugly scar over the mid-right rectus muscle with two small openings at the upper border of the incision about one inch apart from each other, discharging a sero-purulent material. There were no masses or free fluid, no tenderness or spasm. There was, however, a marked post-operative hernia.

The main possibilities as to the causation of these discharging sinuses, i.e., foreign body, in

testinal tuberculosis and fecal fistula were considered. Because of the history of an operation of six years duration with prolonged drainage, the possibility of fecal fistula was foremost.

With the co-operation of Dr. Frank Maloney, then a roentgenologist at the Boston City Hospital. X-rays were taken before and after the injection of Beck's Paste into both sinuses. The plates prior to the injection of the paste revealed nothing remarkable, but those taken after the injection showed the paste lodged into the small bowel. Thus, having established the connection of these sinuses with the bowel, an operation was advised, and the patient consented.

On September 17, 1928, the operation was undertaken under ether anaesthesia. After the usual skin preparation the orifice of the sinuses was cauterized with carbolic acid, and a purse string suture was placed around them so as to invert them in order to avoid spilling of contents into the healthy tissues during operation. An elliptical incision was made on both sides of the old scar incision. On entering the peritoneal cavity the bowels were found to be adherent to the abdominal wall. As these were being freed an indurated stump of the appendix was found, about one inch in length which was leading to one sinus. The stump was removed in the usual manner as for an appendectomy. As the process of freeing the remainder of the bowel was continued, the second sinus was found to lead into the ileum. The tract of this sinus was removed and the rent in the bowel was closed. Then the old post-operative hernia was repaired and the abdomen was closed in layers without drainage.

The patient made an excellent recovery, the wound having healed with primary intention. The patient was discharged from the hospital on the twenty-first day after admission.

Seven months have elapsed since the operation and the patient reports a good scar, a gain in weight of ten pounds and the disappearing of her nervousness.

SOCIETIES

Providence Medical Association

The regular monthly meeting of the Providence Medical Association was called to order by the President, Dr. Arthur H. Ruggles, Monday evening, June 3, 1929, at 8:50 o'clock.

A letter from the council on Medical Education and Hospitals of the American Medical Association regarding hospital privileges and medical society programs was read and no action taken.

The Standing Committee having approved their applications, the following were elected to membership: Charles J. Ashworth, George Edward Clark, George Raymond Fox and Arthur P. Noyes.

Dr. Henry J. Hoye read an obituary on Dr. John F. Gannon and it was voted to spread this on the records, send a copy to the family and publish it in the Rhode Island Medical Journal.

The first paper of the evening on a case of Undulant Fever in Rhode Island, was read by Dr. Niles Westcott. He reported a case of fever in a young man which agglutination tests showed to be Undulant or Malta fever. The patient had been working among cattle. Undulant fever, enlarged spleen, joint pains, profuse sweats and relapses characterize this disease which also affects some domestic animals. The paper was discussed by Dr. Wing, Professor F. P. Gorham, Dr. George S. Matthews and Dr. Westcott.

Dr. Westcott reported a case of heart disease in a man who died in the fifties after suffering from the age of fourteen. The heart was exhibited and had double lesions of the aortic and initial valves. The patient also had pericarditis.

The second paper was by Dr. Frank M. Adams on Traumatic Mastoiditis. He reported two cases treated by him of mastoiditis following injury where incomplete X-ray work had been done and consultation delayed. Six other cases had been called to his attention. He felt that every head injury should be seen by the neurologist and the base of the skull and particularly the mastoid X-rayed. The paper was discussed by Drs. L. B. Porter, Gerber, Hoye, Gifford, Van Benschoten, Cutts, Corvese and Adams.

Dr. Wing suggested that a microphone and horn be placed in the hall to help out the acoustics. Dr. Harrington discussed this as did Drs. Partridge, Chase and Gifford. It was voted to refer the matter to the Building Committee of the Rhode Island Medical Society.

The meeting adjourned at 10:30 P. M. Attendance 60.

Collation was served.

Respectfully submitted,

Peter Pineo Chase, *Secretary.*

OBITUARY

JOHN F. GANNON

Doctor John F. Gannon, a member of this society, died suddenly in his office, 1955 Westminster St., on February 26, 1929, of an acute heart condition, probably coronary occlusion.

He was taken suddenly ill while talking to a patient and died before medical aid could be summoned.

Doctor Gannon was born in this city in 1876, educated in the city schools, graduating from Mount Pleasant Grammar School and La Salle Academy. He was then employed by the Western Union Telegraph Company, the Postal Telegraph Company, and the New York New Haven and Hartford Railroad Company.

Deciding to take up the study of medicine he entered Cornell Medical School, graduating with the Class of 1904. He then served an internship at Saint Joseph's Hospital, Yonkers, New York.

Returning to Providence he established an office in the Olneyville section of our city where he has practiced since 1906.

Doctor Gannon was a quiet unassuming man of exemplary habits and devoted to his profession, and to his brother and sisters with whom he made his home.

He was especially kind to the poor and gave freely of his service to their care.

He died as he had expressed a wish to die when his time came—at his work.

JOHN G. WALSH, M. D.
HENRY J. HOYE, M.D.
WILLIAM H. MAGILL, M.D.

HARRY WINFIELD SMITH

Harry Winfield Smith was a practioner of medicine in the town of Scituate for thirty years. The area of his practice included the towns of Scituate, Foster and parts of Smithfield and Johnston. The population of this district was between seven and eight thousand. He shared this territory with three or four other doctors. The distance between the extreme points of his calls may have been fifteen miles. This was before the City of Providence obliterated farms and villages of this locality that it might supply its people with water.

He began his practice with one horse, but soon he was using two and later three horses. Those were the days of stony and muddy roads. For nearly twenty years he knew nothing of the comforts of the automobile and the macadam road. He missed nothing in the way of weather. He faced the driving rain and the biting sleet and snow. On a cold winter's night when unable to find shelter for his horse, he would hitch him to a fence or tree; cover him carefully with a heavy blanket; kick away the deep snow from around his horse's legs, in case he had no shovel, pat him on the neck and leave him there—perhaps for many hours. He loved his horse and often pitied him. On a dark night and all alone, if your horse fell on the icy road perhaps you could free him from the tangle of harness and broken shaft; get him up; tie the harness; splice the shaft and drive on. Dr. Smith had to do it.

To us of today the compensation he received for his work seems small. For more than half of his professional life ten dollars was the fee for an obstetric case (easy or difficult) one dollar for a house visit, and fifty cents for an office call with no additional charge for medicine. Yet he provided well for his family and gave his son an expensive education.

It may not be realized how much time the country doctor of thirty years ago had to spend in preparing his medicines. Dr. Smith had to drive about eight miles to the nearest drug store. The country doctor bought his tinctures, fluid extracts, syrups and chemicals, and compounded his prescriptions; put up powders and even made pills. Then, as now one could buy empty gelatine capsules and also hollow suppositories. Into the suppositories he would put the powdered morphia; fit on the cap and melt the two parts together with the heated blade of his pen knife or a heated wire. He, like other doctors rolled his own bandages. If a plaster of Paris bandage was needed, he made it. Many at this time made their own splints. A very good splint was made from sole leather immersed in hot water and fitted exactly to the part. All this made much work for the doctor.

To Dr. Smith country life had its compensations. He liked the genuineness of the people and the neighborly contacts. He loved nature and its ever returning beauties.

He was born in Auburn, Maine, August 31, 1867. He received his elementary education in the city of his birth and graduated from the Edward Little High School. He took one year at Bates; then entered Tufts College and graduated in 1890. He received his medical diploma from Harvard in 1893. He first located in Providence, stayed there two years, then took an office in Rockland, Rhode Island, and after a few months moved to the neighboring village of Scituate, where he spent the remainder of his life.

In June, 1895, he married Flora A. Hackett of Auburn, Maine, a classmate and companion of his early life. He was medical examiner for Scituate and Foster; health officer for Scituate, and a trustee of the Scituate Public Library, in which he took great interest. He was a member of the Masonic Lodge of Greenville; of the Scituate Chapter and the Providence Masonic Council. He was a fellow of the Providence Medical Association; The Rhode Island Medical Society and The American Medical Association.

He had an only child, a son, Merritt P. Smith, a graduate in the civil engineering course of the Massachusetts Institute of Technology.

Dr. Smith was a Republican in politics. Like a good citizen he voted but took no great interest in party politics.

He had no particular hobbies. He enjoyed working in his garden, and at times he did a little carpentry and repair work. His chief pleasure was reading, not necessarily medical reading but of general literature as well. He loved his books and enjoyed reading aloud to his wife in the seclusion of his home.

In the sickroom he was very quiet. His answers and explanations were brief. His calls were strictly professional. He was kind and devoted to his patients, who became much attached to him.

Dr. Smith had been ill for nearly five years with chronic nephritis. He passed part of the winter of 1926 and 1927 in Florida, in the hope that he might check the progress of the disease.

He bravely continued his practice, saying very little about himself. The morning of his death, December 24, 1928, he made a house visit before breakfast and attended an office patient. As he sat at breakfast he said that he could not see; he became nauseated. He said to his wife, "I am very sorry to interrupt your breakfast." Those were his last words. He was immediately seized with convulsions and died in about three hours.

Those last words of thoughtfulness for another were characteristic of him. His whole life was spent thinking and doing for others. He died loved by those closest to him and respected by every one who knew him.

IRVING S. COOK,
J. E. MOWRY,
H. G. PARTRIDGE.

CALCIFICATION OF VESSELS IN DIABETES

Roentgenographic evidence of calicification of the vessels was present in the legs in 53 per cent. of 324 diabetic patients varying in age from 2 to 81 years examined by L. B. Morrison and I. K. Bogan, Boston (*Journal A. M. A.*, April 27, 1929). In the third decade 6 per cent. of the patients showed vascular calcification, and in the seventh decade 87 per cent. Twenty-one per cent. showed advanced calcification. Advanced calcification was not found under 40 years of age, although definite calcification was present in five cases. No case of gangrene was found under 40 years of age. Syphilis, dental infections and arthritis appear to play no part in the production of vascular calcification in this series. Seventeen per cent. of patients with calcification (average age, 41 years) and 49 per cent. of patients with calcification (average age, 59 years) have blood pressures over 150. Patients over 50 with sclerosis are about twice as apt to have high blood pressures as those without calcification. Six, or 9 per cent. of the patients with diabetes of ten years' duration, did not show calcification. Morrison and Bogan conclude from these observations that the incidence of vascular calcification increases with age and with the duration of the disease, and is higher in diabetic than in nondiabetic patients. The degree of the calcification increases as a rule with age and duration. The blood pressure in this series of diabetic patients increased with age but not with duration Roentgen examination is an accurate method of judging the presence of calcium in the vessel walls. It is more reliable than clinical methods in the diagnosis of vascular calcification. Roentgenograms of the extremities would be helpful to insurance physicians. Diabetes mellitus is an etiologic factor in the production of vascular calcification.

TEACHING OF MEDICINE

Ralph H. Major, Kansas City, Kan. *(Journal A. M. A.,* April 27, 1929), in a paper read before the Annual Congress on Medical Education, Medical Licensure and Hospitals, Feb. 18, 1929, says that a physician should be not only a doctor but an educated man, and that the standing of the profession will suffer if we turn out a lot of doctors who have galloped through the college and medical course. The degree of M.D. should indicate a certain educational and intellectual status. It means that the possessor of this degree has slowly and often painfully trudged up the difficult and winding path of knowledge until he has reached a certain summit—not that he has taken the funicular and arrived there in half an hour. Major says that he tries to teach the student the technic of taking a history, making a physical examination, carrying out the simpler laboratory tests and then, after these details have been mastered, of proceeding to the diagnosis and treatment. The physician should first know the symptons manifested by a patient even if the diagnosis is not apparent, for once he is sure of his observations he can, by reference to his books, usually make a correct diagnosis even when the disease is one of which he has not even heard. Medicine is not learned by diligent cramming over the midnight oil but by what one might term the "episodic method"—teaching of medicine as a series of episodes, at first perhaps unrelated, but later assuming the appearance of a connected story, or a well rounded experience as the episodes multiply. There is no doubt that medical students, like many physicians, attach undue importance to the laboratory. While we stress clinical instruction in medicine, if necessary, to the partial curtailment of didactic courses, we believe the latter have a limited but definite place in the medical curriculum. The question of the proper textbooks in medicine is a constantly recurring one, and to both student and teacher a very complex and often perplexing problem. Medical books are to be used as reference works and not as repositories of medical dogma which must be memorized and defended like the catechism. An effort is made to show that the clinical picture of a certain patient is the thing to fasten first in one's mind, and then one's knowledge may be extended by reading the composite picture of this disease presented in the textbook and noting wherein the condition of this particular patient resembles the usual picture and wherein it differs. Students should be encouraged to read good medical biographies. This historical method has a great teaching value. One lesson to impress on the student is that the patient consults him because of pain or discomfort, and that if a cardiac patient is seen in the late afternoon it is more important at that time to give the patient a comfortable night's sleep than to learn whether he has a mitral stenosis or aortic insufficiency.

DEDUCTIBILITY OF TRAVELING EXPENSES CONFIRMED

The Commissioner of Internal Revenue has acquiesced[1] in the decisions of the Board of Tax Appeals[2] in which the board held that a physician in computing his federal income taxes may deduct as a professional expense the reasonable cost of travel including railroad fares, Pullman accommodations, room and board, incident to attendance at meetings of medical organizations of which he is a member. The commissioner's acquiescence marks the end of the controversy by a decision in favor of the medical profession. To those physicians who in 1926 and thereafter paid increased federal income taxes because of the decision of the Commissioner of Internal Revenue in 1922 denying the ductibility of traveling expenses, the way is now open to obtain refunds of the excess paid. Presumably, however, the amount claimable as a refund will ordinarily be too small in individual cases to warrant the trouble incident to obtaining repayment. If the amount involved is sufficiently large to justify a physician in applying for a refund, he should obtain from the collector of internal revenue to whom the excess taxes were paid a copy of form number 843, execute it before a notary public or other officer authorized to administer oaths, and file it with the collector to whom payments were made. No application should be made, however, unless the physician is prepared to verify his claim by adequate evidence, in addition to his own affidavit, if called on to do so. It is presumed that the Commissioner of Internal Revenue will exercise a reasonable discretion, with respect to such demands, but nothing will be gained by submitting claims that cannot be reasonably well verified.—*Jour. A. M. A.,* April 6, 1929.

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[1]Internal Revenue Bulletin 8:12-1 (March 25) 1929.
[2]Cecil M. Jack v. Commissioner, 13 B. T. A. 726; J. Bentley Squier v. Commissioner, 13 B. T. A. 1223.